

Anonymized Open Ended Questions

The tables below contain the open-ended question responses to the Psychologists Survey. The order of these responses has been randomized so they do not correspond to each other or to the order of the anonymized dataset. These responses have been lightly redacted, removing some specific examples and other comments that may have allowed the identification of participants.

Changes to Research Practices

Open Ended Responses

What are the most important changes you have made over the last 10 years to improve the quality, rigor or robustness of your research?
Preregistration, careful attention to measurement, collaborating with people who are better at research design.
I pre-register all planned studies, share open data and materials (where possible), and keep detailed commented code in R for all data cleaning and analyses to ensure I (and others) can replicate my research in the future.
I've gone the way of Cronbach- description over explanation. And perhaps more focus on logic over empirics in many cases, especially in measurement. [REDACTED - potentially identifying]
Moving to pre-registrations, registered reports, data and materials sharing, higher-powered studies, behavioral data, no more p-hacking, etc
Preregistration, data and script sharing, reproducible workflows, estimation stats (effect sizes, CIs, etc.), and Bayesian stats, but I think all of these are secondary to rigorous theorizing.
using pre-registrations, making analysis plans and hypotheses public before running the data collection, publishing code and data
Preregistering hypotheses and analysis plans, open script files always (also data preparations), open data when possible, large-scale collaboration (only once and a minor role), focusing on sufficient power and doing power calculations, controlling error-rates for p-values
making data publicly available, reporting null findings
Not building on others' research unless it's been replicated

Pre-registration

Learning R

Developing a better understanding and skills in data cleaning and wrangling

Independent, post-study learning about statistics.

Use larger samples + power analyses

Sensitivity checks

I have started using principles of open science more commonly. The biggest barrier to this in the past was the standard approach we would use for ethics forms where we told participants that we would destroy their data after 5 years, which we were legally obliged to follow. However, more recently, we now state explicitly in the plain language statement that data will be deidentified and subsequently uploaded to the OSF.

Preregistration, larger sample sizes, stronger empirical justification for hypotheses

Increased Transparency in reporting methods and results.

Pre-registration of methods, hypotheses, and analytic strategy, as well as commitments to publish whatever we find (even if only as a preprint).

I wrote one paper as a registered report. Have tried control for multiple comparisons in the analysis in some papers. Are more mindful of sample size than before. Try to use large data sets.

Preregistration, post analysis code and data online, share all research materials, robustness analysis, self-replicate

pre-registration, increased sample sizes, sometimes doing self-replications

Pre-register, open data (make data open & available for replication studies), write more "carefully", drawing less sweeping claims from findings

Continually learning about open science and transparent research practices and following best-practice guidelines. Pre-registration of all empirical studies.

Better stats, method transparency

Posting all materials and data analysis plans (including a power analysis with screenshots of the G*Power parameters) on the Open Science Framework before data collection has resumed; being transparent about all exclusion criteria and if there were any changes; reporting effect sizes in addition to significance statistics; including more analyses that are meant to replicate published findings; make more decisions with other researchers rather than alone

Preregistration and larger sample sizes

transparency of procedure, consistency in data analysis (sticking to guidelines, standards and/or original plan)
Personal collection of samples in smaller population
clearer exclusion rules and checks in my studies. studies don't go in paper unless i've replicated on my own (hence the discrepancy in my ratings on last 2 q's)
Improve design and planning. Transparency in planning and disseminating results Act in good faith and high integrity as much as possible Pre register if possible. Elegant/simple studies. Multiplicity corrections if sets of tests. Report tests that are not significant and all related stars for reproducibility. Open science, open data, open methods, different or non partisan peer review process.
Massive increase in sample size. Both a priori power analyses and sensitivity analyses for all current studies. More sensitive to running multiple covariate analyses looking for something that reveals $p < .05$.
Pre-registration, a priori power analyses, higher statistical power
I pre-register my studies, I do not delete extreme cases anymore and have been more careful with data collection and data report
Pre-registration, higher-powered studies, more informative control arms.
Much higher sample sizes, open data and materials, replicating multiple times myself before trying to publish (when possible)
Hypotheses, methods, and data analyses are pre-registered almost 100% of the time. Data, code, and stimuli are open source by default. Experimental pipelines have been formalised and scripted.
Sample size.
[REDACTED - potentially identifying]
We typically pre-register our studies, whether or not we pose hypotheses. This action has improved the way we think about conducting our research more than making anything more reliable. We have improved our study power, but we didn't really have a problem with that. We religiously directly replicate our work when we can.

Adoption of pre-registration practices for all new programs of research. In addition, I no longer seek to publish one-study papers, except in special circumstances. My current practice is for exploratory analyses to be replicated in pre-registered follow-up studies prior to publication.
pre-registration; increased sample size
Pre-registration of hypothesis
Almost always using preregistration = RR format, sharing everything (data, code, materials...), doing sensitivity analysis, participating on replications, learning new methods...
Proper calculations to determine required sample size. Preregistration of hypotheses, design, and analyses and distinguishing between confirmatory and exploratory results. Materials, data, and code sharing.
Pre register. Not running a bunch of analyses.
Posting data, diff analytic choices to avoid phacking, exploratory and confirmatory studies
Power analysis, replicating others work while doing my own work, writing out pre registration plans with standardized sets of analyses and decisions.
More reproducible code, resisting lower power analyses especially postdoc, using hierarchical models to better capture sources of variation and for regularization
More use of pre-registration and registered reports. More replicating a study before publishing it.
Preregistration for everything; specificity in analysis code, etc.
We preregister far of our work, as well as publishing data and code. We have implemented procedures for clearly specifying deviations from preregistrations, considering validity-severity trade-offs. All manuscripts now includes dedicated sections on transparency and constraints on generality. More of the studies we are publishing are multi-sample with internal replication and probing for generalizability.
Conducting a priori power analyses and powering studies appropriately; pre-registering studies and primary outcomes in particular.
Preregistration Power analysis and Increase statistical power by several factors Making data and code available as standard procedure Preprint sharing online

<p>Ask independent consultant to reproduce results before publication (for complex data analyses)</p> <p>Training graduate students to incorporate evidentiary value in all the research they do with me</p> <p>More thorough articulation of theory</p>
<p>Preregistration via standardised forms, open data and code</p>
<p>Pre-registration</p> <p>More study design feedback before running studies</p> <p>Simplifying data analysis (i.e., R script) for reproducibility</p> <p>Replicating findings in multiple studies with varied stimuli within a paper</p>
<p>pre-registration, power analysis, adding more robustness checks, discouraging p-hacking</p>
<p>Using new methods that account better for error, considering equivalence and collecting better community data with longitudinal methods</p>
<p>Pre-registration and a priori power analyses</p>
<p>Pre-registration of statistical analysis. Attempting to only do well-powered studies (which funding being a major constraint here)</p>
<p>simulation for power calculation,</p> <p>more personal transparency about p-hacking</p> <p>model computational models, fewer linear models</p>
<p>Pre registration, larger sample sizes</p>
<p>Statistical power analyses prior to running studies and pre-registering roughly 10% of the studies I conduct. My most important change is to assume that the effects I study are rarely larger than $d = 0.20-0.40$.</p>
<p>Increasing power prediction when appropriate. Moving toward statistical procedures that emphasize the nature of the data more appropriately (e.g. never using ANOVA in quasi experimental; designs)</p>
<p>Broader research from several authors and papers</p>
<p>Preregistration and being rigorous with correcting for multiple comparisons</p>
<p>Reading more relevant sources b</p>
<p>preregistration, sample size increase</p>
<p>Pre-registrations, much higher power, no p-hacking.</p>
<p>Power calculation, larger sample sizes</p>

Robustness checks, code availability, systematic code testing
pre-registration of hypotheses and analytic plan. cross-replication in additional cohorts if available. sensitivity and robustness checks. published missing data handling in studies.
Increased power
Pre-registration
Not p-hack Pre-register Be honest about exploratory analyses
I learned more about statistics and methodology
King
Preregistration, large samples where possible
More documentation of the research process and the output. Share as much as possible on Open Science framework. Have tried pre-registration with mixed success
Pre-Registered, Model testing rather than looking for correlations
Working with a highly professional research team. Keeping up to date with research in the field.
Pre registration of some, publishing code, improved sampling
preregistration, open data/methods
Rigor in data collection. Ensuring responses are serious.
Mutiple comparisons adjustment, pre-registration. I now have access to more funding and resources, so I can power my studies better, do more piloting, and worry less about one study failing.
Stopped p-hacking. Increased sample size. Theoretical re-alignment such that I don't study things that are too implicit, or too inexact to be meaningful (eg people "tend" to do X)
Pre-registration of studies, following publishing quality checklists,
Learned to implement open science principles to make my findings more robust and replicable (open data, open scripts, pre-registration, ...)

I have used only previously used questionnaires and surveys that possess high validity and reliability factors, such as the "Parental Burnout Assessment"
Pre-registration of analysis and a commitment to publish results regardless of whether they are positive or negative.
Conducting power analysis; learning advanced statistics
Get involved with open science initiatives
I have begun my training in the last 10 years, so have grown from nothing to a competent researcher
Conducting replications of my own work, power analyses for sample sizes, conducting research that are not randomized controlled trials but in a setting outside of the lab and pseudo experimental sampling studies.
Consuming research from non-WEIRD markets. Building an AI companion that's smarter than me in some methods.
Becoming more educated on proper and improper analysis techniques. Pre registering studies and making data/analysis code available.
Preregistration, more robustness checks, made all data and code available
theoretically grounded, methodologically sound
Grading the responses basis time lines. Increasing sample size and sample diversity Randomized tests
I started caring.
I'm still learning, but power analysis has become a big concern in the field, as well as reproducible stimuli and publicly available code written in R or a similar command line environment. This being said, power and high p-values alone can't really address the replication crisis. As an example, you might have to account for the effects of studies being run by the same labs, also sometimes in the same or similar populations. I've gotten into studying more rigorous quantitative techniques to be able to keep up with best practices, but it's a big demand on new grad students and many aren't able to keep up with new methods to the extent that it is probably needed.
Consulting with colleagues, considering improvements for increasing the diversification of samples, improving construct validity

Colleagues' Research Practice Changes

Question: If your colleagues knew that there was a substantial chance that a highly visible replication study would be conducted on their papers a short time after publication, how likely do you think this would be to change how they conduct their research? (Likelihood scale)

- Not at all likely (coded 0)
- Slightly likely (coded 1)
- Moderately likely (coded 2)
- Highly likely (coded 3)
- Extremely likely (coded 4)

Open Ended follow up question: Please explain:

Open Ended Responses

Numerical Response:	Please explain:
2	I really don't know- and the looming AI impact makes me even less confident- it could push it either way. But I think it will change social science research more in the next 10 years than the replication crisis has in the past 10.
1	Most don't care
4	People don't always hold themselves to a sufficiently high standard on their own. By knowing a highly visible and hopefully rigorous and carefully conducted replication will be run, and assuming they care about arriving at the truth and the quality more than the quantity of their publication record, then I'd predict they would set a higher standard for their work.
2	I think they'll still get a lot of the attention for their findings, and a lot of credits (status, future career etc) for a publication in a good journal, even if the replication would show the finding does not replicate. The replication might not get enough attention, so they will care a bit, but not too much.
3	Double and triple checking everything. Sure to preregister with detail. And differentiation on whether the result comes from a confirmatory analysis or not.
2	This impact will increase with time as it becomes normalised and well-known and older generations leave.

1	We generally use the simplest valid analysis and conduct meta-analyses where we make our data and scripts publicly available on the OSF, and so this concern is a bit less relevant for us.
3	I think researchers would get larger and more diverse samples, be more careful about how they operationalize their variables, and be more likely to include the simplest valid analysis before complex analyses.
3	People would not want their work to be immediately "disproven"
3	It's a bit like the difference between thinking that a random amount tax reporting gets spotchecked and knowing your taxes in particular are being eyed for audit.
3	Loads of my colleagues think the final product is the publication; few thing longer term about the paper's replicability — that's seen more as a nice to have than a need to have (for existential matters like hiring and promotion). When I think about writing for replicators, I become far more detailed in describing my methods and far more likely to hedge about the confidence and importance I attribute to the result(s).
4	Probably they will self-replicate more if they haven't done so before
2	Pre-registration has become more common but deviation from pre-registration is also fairly common. I think knowing this might force more researchers to spend more time on the methods and analysis plans before conducting their work.
2	It's a double edged sword; people who are doing their best might still be nervous of not doing enough or being dismissed because of an honest mistake (I mean, we're still humans and should be allowed to make mistakes, if we're willing to correct them and learn from them). But people who are sloppy because they don't care for good science but just want to publish and advance their careers, or actual frauds, maybe they will change as well. Or they'll find other loopholes...
1	I am aware that my colleagues have also embraced open science best practices, so they should not be perturbed by the idea of a replication attempt for their work.
2	They might make sure that all elements are available to sufficiently replicate the study, either in the formal writeup or in a supplemental material link. They should have materials already available rather than only having them available by request from the author. They might make sure to preregister their main hypotheses.
1	More care and precision

1	I think most researchers are pretty confident that they are doing their research properly and would therefore not fear future fail to replicate studies. But they might reexamine their methods if they knew a replication study would follow and perhaps tweak their methods a bit.
2	It's all about the incentives, which are limited to publications and grants. If they can still publish their research in high-impact journals although the replication attempt failed, they probably won't change their behaviour.
3	Observer effect , the mere sheer presence of authoritative stance would crack spirit and might force them to p- hacking as good "doctorial" academic, not a scientist.
3	Because of the lack of nuance quick headlines provide ("X paper by Y scholar didn't replicate! - must be fraud/phacking/etc"), my guess is people would engage more in the mechanisms that are *supposed* to safeguard against that - preregistration, high sample sizes, etc. That way, they can always say "it was bad luck" rather than "it was p hacking". Not sure the change would be in the best faith though - e.g., multiple pre-registrations.
2	I think that it would affect them, but it's hard to know how much everyone would care about the replication project and it's findings. Not sure how easy it is for you guys to get status in the field
1	Confidence is low because I am usually the methodologist and statistician (and I usually agree with theory/hypotheses or suggest improvements, before the study). Also my colleagues seem like honest ones. At least one emeritus distinguished professor disclosed to me that they would conduct research differently if they could do it over again (they might have p hacked or HARKed a moderate amount).
2	No one likes to be wrong, so it would likely motivate greater care in the conduct of the original research. But, I do not envision a world where this is a credible expectation. Most papers published in the top journals of my field are too involved with too many studies to be directly replicated in any reasonable time frame.
2	More accountability
4	He would be more careful before doing something "wrong" like fraud, p-hacking, and so on
4	Research in social psych has become so niche and overly complicated, people can count on others *not* attempting replications. No one likes to deal with

	failed replications, so knowing a replication was coming might lead researchers to slow down (not rush to publish before they're sure of an effect) and use simpler analyses that make it clear whether an effect is there.
0	Let me preface this result by stating that this survey seems to presume that psychological research is monolithic, encompassing one type of methodology - the empirical study which relies on NHST in order to test ordinal differences between conditions. [REDACTED - potentially identifying] I would urge the researchers to consider that psychology has a plethora of methodologies, only some of which have the issues that are under consideration here.
2	Those who were p-hacking or similar would be less likely to do so
1	One of my colleagues is full And notoriously data sketchy. I think this policy would make s/he obscure the methods to claim that the replication was inappropriate.
3	I think that researchers would be more clear in their methods descriptions. Also, the idea of replication is interesting but/and is this really what we want? I do like this idea though.
3	My friends who still p-hack are acutely aware of the social hierarchy and understand the reputational cost of having their work disconfirmed. They would be less likely to p-hack if they knew a direct replication was coming.
2	I think knowing that our work will receive that level of scrutiny will make people more cautious, and more inclined to conduct more studies and internal replications prior to publication.
3	it would make them think twice about their methodology and approach to analysis
2	I think more likely to preregister
2	It's easy to explain away results that don't match your beliefs.
2	I think they would try to be a lot more transparent in methods and analysis (e.g., making the code extremely readable and clear)
1	Ultimately, the incentive structure rewards publication and rarely penalizes researchers even when substantial portions of their work prove not to be replicable. Indeed, it is not uncommon for researchers to continue to push past work even in the face of high-powered replication [example REDACTED]
2	Less chance of wanton sloppiness in execution.
2	It does serve a good accountability mechanism

2	The fear/concern of their findings not replicating and having public accountability for this would probably improve research practices. However, I downgraded my response because (unless there's a registered report approved) the researchers should expect a low likelihood of their research getting accepted in a top journal based on the journal acceptance base rates. So if the researchers don't expect to get accepted in a top journal anyway, then that research wouldn't be subject to the highly visible replication study (although a caveat to my caveat is that researchers probably are overconfident about the chance of their research getting accepted in a top journal)
2	They would be more careful to not use p-hacking, etc.
0	I think they would be happy if someone would do that!
0	Insularity. I can't think of an instance where academics paid much, if any attention, to work done by someone outside the academy. The initial reactions to Repligate revealed that very senior psychologists felt entitled to disdain Nosek's major project to replicate 100 studies. Over time, the Data Colada posse has bumped up the level of rigor (esp via pre-registration), but there are still more than 1/3 of APA journals who don't even request that simple step.
1	I think this will only have a modest impact. In the end of the day, researchers mainly care about fancy publications rather than independent replications. If the latter turn out not to replicate the original findings, they can always resort to (meaningless) hidden-moderator arguments.
2	All research is messy and even with the best intentions best practice cannot be followed to the letter. So replication may change lazy decisions, it's unlikely to entirely change a researcher's whole practice.
3	Knowing one's work will be soon replicated can be an effective curb on fraudulent or dishonest attempts, given that the risk of being caught is now perceived to be much higher compared to what we see in the vast majority of cases, where replication efforts are often temporally distant.
2	I think it would actually encourage them to write and publish work, as they could use replication to demonstrate the strength of their findings. They would make sure to provide clear methods so that the studies are replicated. I think it would excite my colleagues
2	I think it largely depends on how the community perceives the credibility of the endeavor to be. If the community perceives the replication project to be highly

	<p>credible (and unbiased), then I think it could serve as a powerful driver of behavior change.</p> <p>ps: as a [REDACTED - potentially identifying] and open science supporter I think this project is a great idea!</p>
2	<p>Most of the top research psychologists I am aware of with previous publication or publications in the top 5 journals know and are aware that others in the field will scrutinize their work and desire to produce work that meets the highest professional and ethical standards and quality in psychology.</p>
2	<p>I think it would encourage better behaviour, but I also think that they could fundamentally conjure *some* reason why results diverged to excuse the results.</p>
2	<p>It would probably change the preparation for the manuscript to include more details about the methodology to allow for the best possible replication.</p>
2	<p>I think people would be more careful, but most people are careful anyway.</p>
3	<p>Everybody is "building the plane as they fly it"</p>
1	<p>There'd likely be more rigor. No one wants to be made to look sloppy or silly.</p>
3	<p>Of course everyone who does research wants that their results are valid and replicable = is a real phenomenon and not just random noise. If it soon would be retested, I believe that researchers would want to do the research with as best methods as possible.</p>
2	<p>Researchers may be more critical in the way they evaluate their own work before publishing</p>
4	<p>Concern for reputation would motivate researchers to be very precise on their studies.</p>
3	<p>Most researchers would want to triple check their findings for replicability before submitting to any journal</p>

Personality Map

Open Ended Responses

We're interested in making PersonalityMap.io more useful to researchers. What features would make it useful to you? What correlations might you use it for in your research? Please share anything that comes to mind.

I would use it for teaching and research on teaching.

Easy accessible, overview of everything searchable, effect size estimates and just spread the word more!

Unfortunately I don't think it would be that useful for me as I conduct and rely mostly on experimental data. I of course understand that some things cannot be studied with an experiment (you can't randomly assign people to a sex or race and some manipulations are unethical, etc.) but in those cases we may just have to accept, as frustrating as it may be, living in ignorance (as opposed to a false sense of knowledge).

An exception I would carve out where it may be useful even for me are situations where a strong causal model exists and causal inference could be used to study some question with correlational data.

I could also imagine finding it useful for comparing to experimental results as a way of better understanding how often and when a correlational/observational effect is consistent or not with an experimental result.

Perhaps multivariate sex/gender differences as examined from an extremely large set of variables/dimensions, not using domain level aggregates such as Big Five.

No log in. Instant access is required to limit friction.

Would find this to be a useful teaching resource

I would like a better way to choose variables such as separate search boxes for each variable I'm interested in, to know the source of the variables (e.g. does conscientiousness come from Soto and John or the HEXACO model), differentiation between single items and constructs with multiple items, the ability to choose variables from a list, and would like the option to see the outcomes displayed in a correlation table (a full table, not just a half table above or below the diagonal).

Search features that show relationships among constructs that at "high"/"low", or above/below a certain threshold. Including the specific items/measures used to measure each construct so it's easier to do content analysis. Correlations within vs across time, as timing can have an effect on the relationships among constructs.

Some way to measure "familiarity with various philosophies" feels extremely valuable but very hard to quantify. For example, I would be very curious to know how well a deep understanding of the tenets of Christianity or Buddhism correlates to certain things, independent of *belief* in those things. Same with different secular moral philosophies.

I'd only use it if it's free and citable via DOI.

Specify which links have not been documented in literature

Sounds like a great starting point to identify whether two constructs that aren't typically researched in the same subdiscipline have any association. If so, then it could be useful for a researcher to spend time trying to integrate theories and form a solid hypothesis which can then be tested empirically.

Im not interested in personality but good work

Providing links to studies that have attempted to establish temporal cause and effect relationships between these variables. I realize that likely doesn't exist for all correlations, but including follow up studies on these correlations (that also perhaps look at other variables) would be helpful to avoid the third-variable problem.

Moral character traits and (meta)cognitive tendencies

I would like to see available correlations for relatively obscure and unstudied constructs.

Difficult to say as simple bivariate statistics in many contexts are biased do to confounding. One should be able to select additional variables.

Behavioral baseline and context amplifiers

This kind of data is not directly relevant to my research, so it's hard to immediately see how I could use it, but I didn't know it exists and may search it for a) curiosity and learning and/or b) research in the future that I don't yet know about :)

Maybe you could market it with concrete examples of how it could be useful, for example?

Clinical and personality if it can be included. I would want to help write applications for it in situ because it could be used as a tool and, with ethical professions, even used therapeutically of used in combination with other data

Ability to download subsets of data for use in exploratory and confirmatory factor analysis. Ability to suggest personality scales for inclusion. Option to run validation studies on new personality measures.
I am interested in variables such as skepticism, regulatory focus, happiness, and altruism.
I typically do not do correlational research but I am interested in thinking about how this information might be useful to my research.
I do more OB work not personality. I use shinybus (I think that's what it's called) but I do support these types of things
demographics? cultural differences?
Include a standard set of outcomes also. Then your data could be used as a reference for future work--e.g., others can report findings from your data as part of their research and then build on it--identifying moderators or mechanisms, etc.
I'm not a personality psychologist, so this honestly isn't that useful to me. Maybe if I observed a correlation or interaction with a trait in exploratory analyses and wanted to make sense of it? But also if this "study" is just an ad for your product, I'm offended and will be reporting your original email as spam.
Not sure but curious to have a look
Emotional eating and its correlates
Not a personality researcher. Would want to see correlations of other things in a meta analytic framework.
Generalizability and sample characteristics. replicability is essential but psychology as a field needs to mature about going beyond WEIRD samples.
Moral beliefs, generational cohort
I'm not inclined to use personality variables
cognitive variables (IQ, working memory,attention) and neuropsych measures
Correlations with real, observable behavior rather than with self-reported attitudes, perceptions, preferences, or behaviors.
I'd have to see more to answer well.
Making the raw data available
I don't have a clear answer yet at this point.
being able to control for other variables, specify moderators/subgroups

I need to know whether it can predict behavior. Descriptive models are often misleading in their accuracy
Correlations between personality and obesity risk
It would be beneficial if it would fill gaps that meta-analysis don't fill. However, it meta-analysis typically provide detailed analyses that go beyond association (e.g. moderator analyses). Tracing articles used for associations should be transparent and well documented as with standard meta-analysis practices.
Exploring how different personality profiles influence behaviors or social interactions. Identifying potential interventions or strategies based on individual personality differences. The distinctions and similarities of female and male burnout with regards to parenting.
Nothing comes to my mind at the moment.
Looking for potential confounds + multicollinearity issues
historical rates of slavery in the U.S.
separate trait/concept and survey questions
Hard to think as I'm not doing research right now and there would be so many possibilities to do research.
Showing the connections as they tie into values and ethics.

Most Reliable Journal

Question: What journal in psychology do you consider most reliable (i.e., least likely to publish results driven by p-hacking, importance hacking, and/or fraud)?

Open ended Responses

What journal in psychology do you consider most reliable (i.e., least likely to publish results driven by p-hacking, importance hacking, and/or fraud)?
None. I don't trust any of them.
I am not sure this is a helpful question, nor do I have an answer. I think many journals are equally reliable.

Hmm. I look more at authors and editors I think.
Collabra: Psychology
JEP:General, Psych Review, Cognition, Developmental Science, Cognitive Psychology
collabra
Meta-psychology Meta-psychology
Some journals such as psychological science have made a lot of progress in this regard. But I generally favour journals in the educational and organizational psychology literatures. I still think some papers in Org psych. such as Journal of Applied Psych, and equivalent journals in educational psych, have questionable findings. This might not be p-hacking necessarily, since experimental studies are rarer in these fields. But I do think HARKing is a big problem (as well as other QRPs), since most findings are not pre-registered and still are not. Some journals such as psychological science have made a lot of progress in this regard (especially when compared to the stuff they used to publish). But I generally favour journals in the educational and organizational psychology literatures (more close to my field). I still think some papers in Org psych. such as Journal of Applied Psych, and equivalent journals in educational psych, have questionable findings. This might not be traditional p-hacking necessarily, since experimental studies are rarer in these fields. But I do think HARKing is a big problem (as well as other QRPs), since most findings are not pre-registered.
I don't know. Probably something in cognitive psychology.
Journal of Experimental Psychology
Evolution & Human Behavior, Cognitive Psychology, Social Indicators Research
n/a
Papers in diamond open access journals where no one stands to directly profit from the publication.
-
not sure
-
nature, science...
Journal of Experimental Psychology: General
Psychological Science

European Child and Adolescent Psychiatry
Law & Human Behavior
JEP: General
Journal of Research in Personality
Meta-Psychology
Non
Psych Science
I don't know, but I would guess something medium prestigious or non-profit journals like PLOS
Trends in cognitive sciences
Psychological methods
In my field, I think they are all about the same - the top journals share a small pool of the same editors and editorial board members.
Don't know
JPSP
None. Even our most prestigious journals are publishing uninformative or incompetent papers.
Oh wow. I can't think of one. The top journals are still often the worst.
Psychological Review
none
JEP:G
JEP
European Journal of Personality
Recent JPSP has been fastidious, especially the Personality and Individual Differences section. Also "importance hacking" is a dumb phrase-- overstating one's claims does not need a hot new label.
Maybe one of the JEP journals, but not JEP:G as that cares about 'importance'
Psychological Bulletin
PCI RR
N/a

Na
jpsp
Psychological assessment
I don't know
AMPPS
Not sure
?
None
Nature
Psychological Science
Frontiers in Psychology
I wish I knew a good answer to this question
European Journal of Social Psychology
Don't know
No opinion
n/a

Biggest Challenges in the Field of Academic Psychology

Question: What do you see as the biggest problem or greatest challenge in the field of academic psychology right now?

Open Ended Responses

What do you see as the biggest problem or greatest challenge in the field of academic psychology right now?

An entrenched culture of complacency about the field's problems. This is a kind of meta-problem. The problem isn't simply that the field has problems, but that it has a culture of not caring enough about fixing them.

The crud factor. Psychology is much more complex than our current approaches can capture. But after that, I do think the erosion of trust in and lack of understanding of the scientific process is a huge hurdle- in the public and among academics. It's also kind of remarkable how easy it is to get a PhD- you definitely don't need to actually understand statistics or probability. My own understanding is way below what it should be.

Incentives. Everybody wants the A-Publications rather than seeking truth. Give good jobs, reward good open science work

1) Weak theorizing. 2) Non-experimental studies.* 3) Mindless statistics.

* I realize how extreme this may sound, but I might go so far as to argue that all research should be experimental except in a few very narrow circumstances (rare neuropsych conditions, manipulation would be unethical in people but a rigorous study could ethically be run in non-humans or in a machine/simulation, etc.).

not enough focus on socially important questions or descriptive research

Lack of skill/training for working with data. I think more/equally as many issues with replication/reproducibility are due to errors in data processing and analysing than p-hacking

A lot of researchers don't understand that hypotheses can't be changed retrospectively. I also think the field focuses too heavily on theoretical contributions, especially in top journals, which comes at the expense of applied research.

The politicization of science. Researchers tend to look at and/or report variables that support their views instead of testing variables that show the pros and cons. They also tend to look at short-term outcomes instead of long-term outcomes, which may be very different after someone regresses to the mean after any sort of intervention. Also with politicization, even if the results are a true effect with practical implications, much of society will reject it or intentionally do the opposite, making what we do irrelevant and little more than self-aggrandizing.

Lack of transparency in publications. Pressure to publish only significant findings and journals prioritizing statistical significance when accepting publications. This goes against statistical logic and needs to be stopped. If the incentive for continuing in the job is getting more publications, and the requirement for publications is statistical significance, then the field is incentivizing practices where researchers will "search" for significant findings, leading to p-hacking, marking, changing analyses, etc.

Peer review is overall very broken.

Imp hacking

Incentives. Flattery and positive results are often necessary to get jobs, keep jobs, and advance. No one can do well in academia by showing only null results or by showing how their colleagues' research isn't as good as that colleague (or their employer) wants us to think it is.

And incentives will probably never prioritize quality over quantity because of public pressure to produce. And null or else critical results will often not count as producing anything of value.

The pressure to publish in journals often leads researchers to cherry-pick results they believe will be deemed publishable. I see this happening all the time. The publish or perish culture is really bad. In social psychology, sociology and gender studies there also political bias that prevents good research from being published.

too many studies being published

Measurement crisis

There is still a heavy emphasis on publishing in academic's career trajectory. This results in a pressure to publish, which may result in conscious or unconscious questionable research practices to ensure that a study is "worthy" of publication. The goals of a journal are to "sell" articles, and therefore they might favor results that are "groundbreaking", "novel", or otherwise extraordinary. Thus, the goals of a journal may compete with the goals of a scientist with integrity. Meaning, a scientist can conduct a series of studies following all the recommendations for transparent science, but then those "dull" results won't ever see the light of day if a journal won't accept them. If journals only accept significant or important findings, this creates a pressure on scientists to produce these findings in any way they can.

Construct validity

Coming up with good ideas with broad significance to test. The field continues to be dominated by the narrow, personal interests of researchers that have little scientific importance. And sometimes these narrow interests are ideologically motivated. As Robert Hogan once said, pick an important topic, and psychologists have not studied it. Or as James Knox Millen wrote in his book, *Your Nose Knows*, "The mountainous research laboratories of the country, staffed to the top with Ph.D.'s, labor and bring forth microscopic mice."

as in science overall, the incentives will always motivate people to hack good science

Focusing on non sense , wasting time , avoiding practicality , lost of identity , leaning toward "therapy" , not real science , at the mercy of medicine, losing glory days of advancing psychoanalysis, lose of creativity and bounded by hierarchal bureaucracy

Frankly, I somewhat resent the "importance hacking" idea. I think it leads to higher rates of p hacking and researchers putting studies up for publication when they aren't 100% confident in results, because we are incentivized to show large societal effects. The reality is that there are hidden moderators everywhere, and that every effect we study is going to be more nuanced than it can be presented in a paper. I think well-done research (e.g., the authors have replicated and have utmost trust in the results - given their study contexts), so long as there is discussion of when and where it is relevant to furthering our understanding of psychology, should be the primary metric by which we judge work.

That we value prestige and interesting findings more than discovering the truth

The publish or perish mindset.

Egos causing packing and importance backing. Incentives. Us political climate

Creating the infrastructure so that high-powered studies that can produce replicable research are possible. In Social Psychology, if all studies are going to need $N = 100$ per cell of an experimental design, that greatly narrows the number of researchers who are going to have the resources to pursue such research.

Incentives that reward researchers who prioritize quantity over quality

How tenure decisions are made and the stakes for the career

Little or no attempt is made to falsify the hypothesis, so we have a great mass of manipulated studies where the authors turn themselves inside out to demonstrate positive results.

Research that is too complex to be meaningful. A push to do "theoretically driven" research when our theories are ridiculously weak. Lack of respect for descriptive research even though many basic phenomena remain woefully under described at a basic level.

This might not be exactly what you're after but academia and academic psychology are under enormous threat due to fascist policies of the Trump administration. Cuts and delays to academic research funding will hurt for a long time to come, especially in the USA. A second factor in less explicitly fascist countries is the push to justify every research decision in terms of its impact to the public. Some research has direct application but other research does not and cannot be expected to given that the outcomes and eventual use of research is unknowable in advance. Third, and probably most relevant to your survey, there is a lack of interest and hesitancy in thinking about probability, statistics, and methodology (particular good measurement properties) amongst students.

Literally zero accountability. [examples REDACTED] No punishment. People want to pretend everything is good and normal.

Gosh, which one? What Trump is doing to freedom of speech has direct implications for academic freedom. Columbia's cave to Trump signaled the end of higher education for me. The holding hostage of research funds. Detainment of our students -- and we're doing nothing. Calling police on students by Presidents. Lack of Boards of Trustees to do their jobs (everyone should be trained by AGB!). Illiteracy of students and lack of help for them. Legacy admits. Etc. etc. etc.

Inertia. The majority of the field has ignored reproducibility issues because the incentive structure still rewards a flashy story over a reproducible finding. As long as this situation persists, there is little hope social science will produce usable findings.

Focus on publication quantity over quality, and scarcity/inequality of resources (funding and jobs) that motivates bad behavior.

The publish or perish model cannot lead to good science. If researchers need to publish to get/keep their position, there is no incentive to choose quality over quantity. Plus, the fact that young researchers are trained by and depend on older faculty who are completely "in the system" does not help.

We have to publish and get grants to keep our jobs so we have to produce something

Moving from verbal to formal theory

Real world data collection of meaningful samples and studies that study meaningful constructs and not just the newest fads

When in doubt, it's not always feasible to conduct a replication study given the time and budget constraints, let alone publish it. In some contexts, even simple attempts to replicate other's works can have real consequences, and researchers may have to risk their career and reputation if they are at the lower levels of academic hierarchy.

We collect "effects" rather than theories.

The pressure to publish and the incentives aligned in a way to exaggerate or falsify results

One important aspect of the survey was covered which is how the field has reacted to the replication crisis. I think open science movements are now much stronger and these practices are slowly becoming more widespread. However, we are still at an inflection point where the cost for the individual when investing in open science practices (i.e. extra effort) are still too high because the system does not yet provide tangible benefits for this investment. Researchers who practice open science believe in it but the system does not yet reward this in a tangible way. However, even if open science became standard practice over night, the issues of the publication system and peer-review remain (this is non-specific to psych). As long as publishing conglomerates hold power over academia, we will always have phenomena like importance hacking. This is my view is not an issue that stems from the replication crisis, but is a systemic issue.

I also recommend the work of Felix Schönbrodt from LMU München. I would say he is one of the most lucid voices the field has in this area making psych science more robust.

There is a lack of professional psychologists among indigenous people and empirical studies from research psychologists.

Lack of standardization. Doctors (should) follow best practices when treating patients, but researchers are given too much leeway in determining random aspects of their research which make them idiosyncratic. It would also be nice if research applicability to the real world held more weight at least in some contexts.

I believe it's the idea that a study cannot be published if it doesn't bring significant results. I think the greater mentality change should be in the direction that if our job is to produce knowledge then a research with null H₀s confirmed could still be an insightful source of knowledge. Also the idea that we have to produce a certain amount of paper for years to be competitive on the job market it doesn't help. Research quality should be evaluated to quantitative indicators (or not solely by those).

It's difficult to get the samples and environments required to properly test a lot of social psychology research

Publish or perish, importance hacking, failures to replicate, teaching students they are experts with a bachelor's degree, file drawer problems, history effects, it's all a big pyramid scheme

n/a

Criticism/Weakness of Open Science Movement

Question: What is your biggest criticism of, or what do you view as the biggest weakness of, the modern open science movement in academic psychology?

Open-ended Responses

What is your biggest criticism of, or what do you view as the biggest weakness of, the modern open science movement in academic psychology?

It isn't forceful enough. Its proponents should be much more critical of how bad the state of the field is, including its complacent culture.

Open science being used in a virtue signalling attempt and derision of people who have yet to take up open science practices, in part due to "bro-open" science attitudes.

The assumption that the problem is actually fixable. Perhaps Meehl was correct and the problem is epistemology, not statistics.

Too narrow and focused and too many egos. Otherwise, big fan!

Goodhart's law. Some of these measures have now simply become targets, with people going through the motions only to appear as if they're engaging in best research practices when they're not.

focusing too much on replication only, maybe

It is too easy to not follow OSF principles. I don't think it should be mandated necessarily, But I do believe journals should ask questions to authors who don't commit to making their data available. This is especially the case for meta-analyses, etc. which use publicly available data and participant consent to share the data is not an issue.

There's too much focus on things that make little difference and can make bad science seem good because it uses open science practices. I can preregister a single hypothesis and get a cute little badge on my paper, but then I can make claims way beyond the preregistration and test additional hypotheses that weren't preregistered. Ultimately, I don't think the typical researcher understands the problem very well so they just follow helpful but limited processes that give the appearance of good science.

Too much focus on preregistration. Sometimes, methods/analyses need to be adapted reasonably after data are collected due to data characteristics, and too many researchers don't know or anticipate this, especially early in their careers. People can also preregister bad analysis plans, then point to them as reasonable evidence for why they did what they did, even if they're using the wrong method or assumptions of the method they used are not met. There needs to be a midpoint between open science and statistical appropriateness.

n/a

Overconfidence or naïveté. I value (and do) open science, but I would be a fool to think open science is a panacea to human vices.

Some people in this movement are quite rude and condescending toward “normies,” which makes it harder for open science to gain broader acceptance.

too rigid? sometimes seems to focus too much on doing things a certain ideal way and if you fall short of it then you have failed in some advocates eyes? not sure, but that's the vague impression I have even though I am a supporter of the movement

Prominent cases in which people who purportedly are at the vanguard of the open science movement behave in ways that undermine open science best practices.

It relies heavily on positivism and ontological realism

All materials and analysis plans should be included with papers that are published as a default, not a recommendation. I understand that sharing data openly may have implications for ethics boards, but those data should still be made available upon request.

Not enough peer checking of preregistrations

There is too much emphasis placed on strictness of methods without considering the fruitfulness of ideas.

to much focused on technical & methodological aspects - which are all relevant as well - while neglecting (not completely) the motivation of researchers to behave the way they do

Avoiding of big claims , not merging with other fields , being an additional non serious field , Therapy not moving fast enough.

I do think there are many cases where researchers do what they can to prevent p hacking or spurious results, and work still doesn't replicate (on a pure or conceptual level; see comments on hidden moderators, or bad luck, or not realizing a confound in study design). Some of these people are understandably fearful of failed replications and possible charges of malicious or bad science. Biggest weakness at

this point: poor branding of these downsides. Sometimes this is open science advocates truly attacking others; sometimes it's just the perception of what their intentions and beliefs are... but it leaves people resistant to fully embracing the realities of what we need to do better science as a field.

Open science has led to an increase in predatory practices by journals, including journals that are not the typical predators, including top journals

Makes fraud more easy.

No criticism. Weakness is that ideals are hard to uphold with financial incentives and prestige/glory

Preregistration is treated as a sacrament that blesses a study with truth value. I do believe it is important to preregister an analysis plan to prevent researchers from trying a variety of approaches to find one they like best. However, assuming that hypotheses must be preregistered ahead of time to make any conclusions from the data valid is saying that you believe in and require precognition. The only circumstances where this matters is when someone is presenting a single study testing a completely novel hypothesis. All concerns about HARKing can be resolved by Study 2.

Sometimes an unrealistic view of how universal and replicable some psychological effects are

I do not see one but I do not think that pre-registration or making everything public solves the problem. Making the original dataset public would make things clearer (the Qualtrics dataset without any manipulation)

It's still too costly in that individuals without university affiliation are unable to engage and valuable contributions are being missed. I wish that papers and reviews/critiques could be submitted by anyone with an inexpensive membership.

The cruelty and rudeness of a few people who take up a lot of air in the movement, older/respected/well known people who just won't get on board.

I think overreach is the biggest issue. There is a failure to understand that different methods have different concerns. Not every empirical study uses a clinical trial type approach and not every study is concerned with generalising its results to populations. There is also less of an emphasis on the need for better theory in psychology. Many studies fail to replicate because there is no good theoretical mechanism underlying their reported effect.

The "bro-pen" aspect. [examples REDACTED] So many conflicts of interests.

[REDACTED - potentially identifying]

Not understanding how entrenched the bad practices were. Not getting the leaders of various fields on board to change. Subsequently, the leaders of fields like social and developmental psychology have persisted in rewarding bad practices by ignoring the proposed changes.

Dogmatic support of methods that may or may not improve the issue, and a tendency to focus on psychology when in fact these practices are widespread in science (looking at you, cancer biology...)

Nothing

So many people already don't believe in science that it weakens trust further in undergrad students when they learn about replication crisis in class

Prestige hacking for tenure and university requirements

An academic culture that values achievement in publishing eye-catching articles, which barely encourages reexamination of older studies. Most of the time we have no choice but to take them for granted, even though some results look very fishy.

Far too dismissive. Sometimes they trust a failed replication more than 10 studies. They often seem to ignore theory in ways that harm the study design, or which make the failed replication trivial. They often assume far too much from a single failed replication. Seem to not care about false negatives.

The difficulty with intellectual property and the ability to turn research into a business idea. Open data and public information makes it hard to develop business

There are too many degrees of freedom when it comes to theories (not many formal theories). This makes falsification very difficult and encourages post-hoc explanation of any phenomenon. This is the part that makes me most cynical of my own field.

Is it the lowering of standards that DEI presents. Merit is the best way to approach the shortfalls. DEI encourages the placement of persons on filling out checkmarks on application forms. There are many very talented individuals among our people. They need a hand up, not handouts.

Replication projects should continue to help the field establish a new foundation that helps the field stand up empirically—right now the field is often taught like a history class, teaching popular ideas but not empirically solid ones.

The Spanish inquisition-like state of mind of some of the researchers that advocates for open science.

Failure to acknowledge that it can be difficult for newer scholars to engage with open science due to time/resource constraints and older scholars are unwilling

All our best efforts today tomorrow will look like our best efforts yesterday today

the fees involved as barriers

Most Valuable Psychology Discoveries

Question: What, in your view, is one of the most valuable discoveries from academic psychology in the past 15 years that you are confident is real/true/accurate?

Open Ended Responses

What, in your view, is one of the most valuable discoveries from academic psychology in the past 15 years that you are confident is real/true/accurate?

I can't think of a single one.

Nothing comes to mind

Gallistel's work arguing against memory storage at the synapse but rather within the cell is theoretically elegant and now has some compelling evidence in support of it. I would place a medium size wager that the central parts of this work, which challenges one of the cornerstones of a century of research on memory and learning (the Hebbian synapse), will hold up.

There is some very cool work showing that cognition (beliefs) doesn't penetrate certain aspects of perception (work initially by Chaz Firestone and Brian Scholl, although it has its roots in work from the 1970s and 1980s by Zenon Pylyshyn), that I think is theoretically important and likely to hold up.

Tetlock's work on superforecasting (especially the impact of teaming, training, and tracking), while still early days, is valuable and I think likely to hold.

I think research around the continuum structure of motivation is fairly replicable, yet the research here is imperfect also.

Research on cognitive biases. They were discovered more than 15 years ago, but the research in this area has taken off in the last 15 years and I think it's very valuable for helping people think better and make better decisions in all areas of life.
Estimating the reproducibility of psychological science by Brian Nosek
Not quite formulated within 15 years, but definitely growing more widespread acceptance: the Self-Determination Theory (SDT), developed by Edward Deci and Richard Rya.
n/a
Loads of stuff from judgement and decision-making (such as anchoring), compared to, say, stuff from social psychology, educational, or clinical psychology.
that economic development increases life satisfaction
maybe research on how people think about and judge misinformation and interventions to steer people away from it
Studies on the power of authority (eg Milgram experiment)
Internalization of appearance ideals is associated with body dissatisfaction.
Decrease in adolescent health Selfinjury as emotion regulation
Structural and systemic influences on intergroup bias (rather than individual influences); the IAT not being a reliable measure of racist behaviors; priming is not a reliable effect
Most true psychological effects are small
That the Big 5 factors capture most of the important variance in personality, and that these factors are related to important life outcomes like mental and physical health, creativity, criminal behavior, and career success and satisfaction.
?
Moving from behaviorism into neuroscience for reasoning
That people do have a substantial interest in maximizing good consequences in moral decisions as long as they don't have to choose between that and other things that they value (think of e.g. giving multiplier and some of josh greene's research)
Moral foundations theory.
Abnormal psychology, especially newer network models of psychopathology and personality

Processes of motivated thinking that are associated with political partisanship.
Genetics of personality/intelligence
I really enjoy regulatory focus and construal theory
Disruption of the reconsolidation phase of a conditioned response as a rapid and robust extinction strategy.
Yikes. The stuff that stands up best is basic persuasion stuff that's older than that. I'm embarrassed if my field that I'm having a hard time answering this.
On a longer time frame, I would have said the applicability of signal detection theory. In the past 15 years, I would argue that the most valuable discovery is that scaling up connectionist/neural network models and training these models with huge amounts of data, allows for highly accurate performance (not necessarily human-like) on many different tasks.
My own work? Empathy gap stuff seems good.
I'm a fan of the naive science work that's been done in developmental psychology. However, I think it's important to say that the empiricists and naive scientists have been honing their findings in conversation with each other and sometimes in the same labs. This breadth of theoretical argument is so important. I mostly feel that much of what we've been doing has been tiny iterations on the same problems without any sweeping new ideas or new areas. [REDACTED - potentially identifying] I think we need to move beyond this.
15 years? That's tough. The most valuable discovery is that most of our work is unreliable and we need to start over.
Growth mindset
Media algorithms promoting outrage bc humans attend to it
Much of cognitive psychology and within group designs
I'm still waiting for more evidence.
New discoveries are rarely valuable because they rest on such theoretically shaky ground. They haven't had time to be debated. It's the refinement of theory in the last 15 years that has been most valuable. eg what can we actually conclude from the marshmallow test, what can we actually conclude from occasional incidences of loss aversion about what this actually implies about human psychology. 15 years ago our understanding of psychology as a science was that we collected effects. This survey continues to treat the field this way
I tend to be appropriately sceptical of many recent findings

<https://journals.sagepub.com/doi/abs/10.1177/0956797620977513>

The field of psychology has an extreme shortfall in minority participation and is attempting to address it.

Much of the behavioural economics research—which should be used to help the field apply the Feynman's principle: the first step is to not fool yourself and you are the easiest person to fool.

the notion that moral judgments are the most important features for group members to evaluate and be evaluated by others.

Worsening mental health

The contact hypothesis

idk